length through a desert in hot weather at the rate of a mile a day.

The other papers in this volume are: No. 1, on "Provisional Fortification," a study of defensive works erected in a moderate time, and capable of extension and improvement, with examples from Adrianople and Tschataldscha. Paper 2, on "Graduated Arcs for Heavy Guns," contains an investigation of the errors in such arcs, and the mode of laying guns correctly, in spite of such errors. Papers 4 and 5 describe some blasting operations in Bermuda. Paper 6 describes bridges laid over the Cabul River during the war in 1879-80. Paper 7 is on "Railway Curves"; and Paper 11 contains "Tables of Ordnance Equipment." ALLAN CUNNINGHAM

---

## *OUR BOOK SHELF*

*Report on the Dyes and Tans of Bengal.* By Hugh W. M'Cann. (Calcutta, 1883.)

THIS Report, which is issued under the direction of the Committee of the Bengal Economic Museum, originated in the efforts made by Mr. Thomas Wardle to collect information on the modes of dyeing the silks of India. This information was asked for so far back as 1875, and although the Indian Government were fully conscious of the importance of instituting an exhaustive inquiry upon the subject, it was not until 1880 that an instalment of the General Report was issued, and from this, for reasons which it is here unnecessary to enter into, the dyes of Bengal were omitted. Dr. M'Cann has doubtless done the best he could with the materials at his command, although there is a probability that the Report would have been fuller and more free from errors had it been possible to put together the information, which was mainly collected in 1875-77, at a time when the officers of the local governments and administrations through whom the information was obtained were still resident in their respective districts. As it is, the Report is avowedly incomplete, and in many points already out of date. The classification adopted is, in the main, the same as that already employed by Mr. Liotard in the Report on "Dyes of Indian Growth and Production" above referred to, but with the difference that Dr. M'Cann has preferred to give the methods of dyeing in connection with the accounts of the dye-stuffs themselves, instead of referring them to the fabrics which are dyed by them. The dyes are classed according to the colour they afford when used singly. One disadvantage of this arrangement is that some dye-stuffs which are used both alone and also in the preparation of compound colours are mentioned several times. Many of the dye-stuffs are called simply by their vernacular names, as they have not yet been botanically identified, and in many of the cases in which the scientific name of the specimen has been given there is nothing to show how it has been arrived at. In spite, however, of these imperfections, the Report adds considerably to our knowledge of the tinctorial resources of India, although it must be stated that owing to the delay in its compilation the original object of the inquiry has been in a great measure lost sight of. The primary object of the inquiry was, in fact, to obtain data upon which to base experiments with regard to the possibility of developing and improving methods of dyeing with native Indian dyes. Dr. M'Cann expresses the hope that this project may be revived. He is of opinion that among the vast number of Indian dyes there are many that might be developed into flourishing industries; but he is equally of opinion that this development will never take place through the native dyers themselves, who are content to follow the primitive methods handed down to them by their predecessors. Dr. M'Cann suggests that great results might follow if the Government would send out to India one or two trained chemists or scientific experts in dyeing to conduct experiments with the special object of developing native dyeing industries. The number of properly trained technical chemists already there is too small to hope for anything from them, nor is it likely that improvements will result from the private enterprise of European firms. As it seems nowadays the fashion to commend all such projects to the notice of the City Companies, it may not be out of place to draw the attention of the Worshipful Company of Dyers to the suggestion. T.

*Lehrbuch der Vergleichenden Anatomie der Wirbelthiere.* Von Prof. Dr. Robert Wiedersheim. Zweiter Theil. (Jena, 1883.)

WE have on a previous occasion (NATURE, vol. xxvi. p. 385) directed attention to the first part of Prof. Wiedersheim's text-book on the "Comparative Anatomy of the Vertebrata," which was published early in 1882. He has now, by the publication of the second part, completed the work, which forms a clearly printed and profusely illustrated volume of 906 pages, with 607 well executed woodcuts. The second part comprises a description of the alimentary, respiratory, circulatory, urinary, and generative organs of the Vertebrata, and the author tells us in his preface that the entire work represents the labour of six years. In his method of treating the anatomy of the viscera, described in this part, he has followed the same lines as in Part 1. The description of the modifications of each system of organs observed in the different classes of vertebrates is prefaced by a short chapter on the method of development of that system, and the subsequent description is then based on their developmental history. We can recommend the book as giving an excellent *résumé* of the subject written in a thoroughly scientific spirit.

---

## *LETTERS TO THE EDITOR*

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

### Evolution of the Cetacea

IN the lecture by Prof. Flower "On Whales, Past and Present, and their Probable Origin," which appeared in your columns in June and July last, he contends for the evolution of these animals from the *Ungulata*, and points to the *Zeuglodons* of the older Tertiaries as predecessors of the *Balænoptera*, and as representing an intermediate stage in such evolution; and he insists on the absence of cetacean remains from any Mesozoic formations as strong evidence in support of this view.

I wish therefore to inquire whether Prof. Flower has considered the evidence afforded by *Palæocetus sedgwickii*, so named by Prof. H. G. Seeley from a set of anchylosed cervical vertebræ (one of which he figures), described by him in the *Geological Magazine* for February, 1865, p. 54.

Prof. Seeley states that the specimens were obtained from the boulder (chalky) clay near Ely, and that they were regarded both by the late Prof. Sedgwick and by himself as derived from either the Kimmeridge or the Oxford clay; and he quotes the opinion of Prof. Owen in the British Association reports, in his "British Fossil Mammals," and in his "Palæontology," that they belonged to an animal of the Dolphin group. Prof. Seeley himself regards this animal, for the reasons he assigns, as "not a cetacean of the Dolphin family, but a true whale, its affinity with the Balænoptera being, he says, singularly close"; and he concludes his description with a letter from a veteran student of the Cetacea, the late John Edward Gray, Keeper of the Zoological Department of the British Museum, who, after pointing

© 1883 Nature Publishing Group

out the characters in which the fossils agreed with, and those in which they differed from, *Balæna*, asserted that in those particular respects the animal to which the remains belonged agreed with a genus of whales which he had just described under the name of *Macleayius*, from a specimen in the Australian Museum in Sydney. Perhaps Prof. Flower regards these vertebræ as not those of a cetacean at all; but if he agrees with the authorities just named on that point, the case seems to resolve itself into this, viz. either this whale lived in Mesozoic times, or its remains have come from some Tertiary formation. If the former, and particularly if its age is, as regarded by Prof. Sedgwick and Prof. Seeley, Jurassic, Prof. Flower's hypothesis of the evolution of the Cetacea from the Ungulates is hardly probable, when we consider the known facts as to the development of that group during the Tertiary period, even if we allow for whatever weight *Stereognathus* may afford of an approach to an Ungulate type in Jurassic times. If the latter, and these remains came originally from some older Tertiary formation, it follows that such a formation has, though no traces of it are now to be found, once existed in the area between Ely and the eastern watershed of the Pennine, because the whole of the material of the clay in which the remains were found is made up of the wreck of formations from that area alone. SEARLES V. WOOD

Martlesham, near Woodbridge, December 6

## "Cosmic Dust"

THE report on Baron Nordenskjöld's expedition to Greenland this year, recently given in NATURE, undoubtedly contains important results as to the physical geography of that country. Its statements, of course, will require a more detailed explanation than this preliminary report can give; one statement especially, on account of its significance, induces me to call the reader's attention to a fact which it will be necessary to take into consideration in discussing the question.

The statement is contained in the following words at the end of the article:—"I hope when this (viz. the dust found on the inland ice) has been exhaustively analysed, to be able to furnish fresh proofs in support of the theory that this deposit is, at all events partly, of cosmic origin, and thereby contribute further materials for the theory of the formation of the earth."

The fact to which I have alluded is this: Next to the observations furnished by travelling over the inland ice, it appears to me that an examination of the fresh and pure fragments of it from the very interior of the country, which are pushed out in the shape of icebergs, must give the best key to the solution of the problem. We know that the mass of which these bergs are fragments is formed of snow accumulated during hundreds of years, and it has taken hundreds of years for the ice thus formed in the central regions to travel to the seashore. Consequently the dust which during the lapse of centuries has fallen upon the surface of the glacier must have been mixed up with the snow, and thereby spread over or embedded in the chief mass that constitutes the bergs.

As to my own observations, I have always found the chief mass that constitutes the large bergs to exhibit the appearance of perfectly pure ice, only permeated with thin air-bubbles, and the earthy matters of the bergs distinctly confined to isolated dykes, layers, conglomerates, or even to entire smaller bergs issuing from certain fjords. But I confess that my attention never was directed to a more minute investigation of the chief berg ice, and still less to the problem here mentioned. I do not remember to have seen anything mentioned by my friends Steenstrup, Helland, and Hammer that could throw sufficient light upon this question. I therefore here present it to your readers who are experienced in Arctic researches and may feel inclined to communicate their opinions upon it.

Christiania, Norway, December 5 HENRY RINK

## On the Incubation Period of Scientific Links

THE length of the dormant period during which a certain class of scientific discoveries has to remain unrecognised before they are made available is a subject that may form an interesting chapter in the history of science. I will cite one or two examples, in one of which I am personally interested, as illustrating my meaning, particularly as I think they will enable me to point out the cause of this strange anomaly at a time when so much attention is being given to original research, and yet which will leave the results of original research to lie dormant for years after they have been realised. As illustrating the fact that most important laws may remain for many years dormant, I have but to cite the law of Avogadro, which remained unnoticed for fifty years, until the investigations of Dumas proved it to be a most important aid in chemical research. The law of Dulong and Petit on the connection between the specific heat and the atomic weight of the elements had to pass through a dormant period of more than twenty years before it was resuscitated by the experiments of Regnault. More than forty years ago I announced a new law connecting the physiological reactions of inorganic substances with their isomorphous relations. This law, although founded on an extensive series of experiments, and since verified by the investigation of the action of the compounds of more than forty of the elements, has up to the present time remained entirely dormant, not having been noticed, as far as I am aware, by any writer on physiology. A French chemist, M. Rabuteau, has recently very cavalierly consigned it *aux baggages du passé*, apparently under the idea that it is a revival of the hypothesis that connected the action of poisons with the more or less acute angles of their crystals. Now, however, the important part played by these inorganic substances as physiological reagents is beginning to be recognised (see Ringer, *Journal of Physiology*, January and August, 1883; Brunton and Cash, *Proc. Roy. Soc.*, vol. xxxv.).

The question presents itself as to what there is peculiar in these laws which distinguishes them from those which find an immediate recognition by men of science. I think the distinction will be found in the fact that these hibernating laws generally form connecting links between two branches of science which had not, up to the time of the discovery of these laws, been of much mutual assistance. The law of Avogadro, for example, established a new link between chemistry and physics, and for its application the chemist had to be familiar with the manipulations required for the determination of the density of vapours and gases, a subject scarcely alluded to in treatises on chemistry at the beginning of the century. The law of Dulong and Petit forms another link between chemistry and physics, requiring for its verification methods which, at the time of its discovery, were almost exclusively in the hands of physicists. As for the law connecting the physiological action of a substance with its isomorphous relations, when it was first published the distance between chemistry and physiology was greater than that between physics and chemistry at the time of the discovery of Avogadro, and should the subject be already attracting the attention of physiologists, after a latent period of but forty-four years, this fact affords evidence that science is now advancing at a more rapid rate than formerly. The question is an interesting one as to the possibility of something being done to shorten the period during which these linking laws remain unrecognised. Offering, as they generally do, important aids for the advancement of science, it certainly is desirable that some means might be taken to prevent their being shelved amongst *les baggages du passé*, so that at some future period the whole subject has to be gone over *de novo*. In the case of physiological discoveries, it certainly would seem to be the duty of the Antivivisection Society to see that the many experiments which had been performed to verify them were made available, so that a great deal of vivisection might thus be avoided without the progress of science being retarded. JAMES BLAKE

San Francisco, November 13

## Meteor

THIS afternoon, at 5.27 p.m., I observed here a meteor of great brilliancy, a note of which may be worth publishing. The moon, within three days of being full, was shining unclouded, and the western sky was still glowing with the fading tints of another gorgeous cloud-glow, when a bright light caused me to look up. It was due to a bright meteor a few degrees south of and below the moon. Its path was about 20° in length between south-east and south, inclined at an angle, roughly speaking, of 10° to the horizon, its mean altitude being probably 20°. Three minutes later, at 5.30 p.m., I heard a low, distant, rumbling sound, which was not improbably the report of its explosion.

G. M. WHIPPLE

Kew Observatory, Richmond, Surrey, December 11

## Physical Society, November 10

UNDER the above heading in NATURE of Nov. 15, p. 71, I notice it is stated that I have found the velocity of sound in air

© 1883 Nature Publishing Group